

THE AUSTRALASIAN JOURNAL OF PHILOSOPHY

VOL. XXX.

DECEMBER, 1952.

No. 3.

REVIEW ARTICLE

REFLECTIONS ON *LOGIC AND LANGUAGE*

By J. A. PASSMORE.

*Logic and Language*¹ is a collection of essays, all previously published, which have been selected by Mr. Anthony Flew in order to illustrate to the general reader the workings of what he calls "the linguistic movement", that philosophical tendency which, originating at Cambridge in the teachings of Wittgenstein, is now powerfully represented at Oxford and in possession of strongly held outposts, as readers of this *Journal* will be well aware, even in the remote corners of the British Empire. Characteristically, this movement has no name; its exponents do not care to be associated with an "ism". Its critics sometimes call it "logical positivism"; that name, however, is best reserved for the teachings of men like Ayer, Schlick, Carnap. Their vigorous generalisations are quite remote in temper from the nice distinctions of the linguistic movement. For convenience, then, we shall give a new meaning to the little-used word "grammaticism"—and its cognates—in order to make it apply to the linguistic movement; the name is appropriate in so far as that movement is remarkable for its claim that the wrong sort of philosophical theory, although in a special sense of that phrase, is "bad grammar" and that the right sort makes clear to us "the grammar of our language".

¹ Blackwell, Oxford, 1951; pp. 206. Price, 16s.

Mr. Flew expresses the hope that collections such as *Logic and Language* will prove to be a method of bridging the gap, at present a notable one, between the general reader and grammaticist writings; and certainly the relative inaccessibility of these writings, most of them periodical articles, does something to explain why names like Wittgenstein, Wisdom, Austin, mean nothing to the ordinary educated man. But I suspect that Mr. Flew underestimates the positive repulsion which many general readers—and a great many philosophers—feel towards grammaticism, as something dreary, tedious and, in the worst sense, scholastic. Certainly, there are two articles in this volume which could not easily provoke this reaction: Dr. F. Waismann's *Verifiability* and Professor John Wisdom's *Gods* (presuming that the educated reader could cope with the anfractuositities of Professor Wisdom's style). But these articles are quite untypical in two respects: they are richly allusive, and they concern themselves with problems which might well perturb even those who are not professional philosophers. Partly because they are so untypical, and partly because they would need to be examined in close detail, I shall say no more about these articles than that they deserve to attract wide attention.

From the other articles, the general reader will learn that his worst suspicions have been confirmed: the philosopher is either a person who falls into verbal traps or a person who spends his life pointing out that *other* philosophers have fallen into such traps. In neither case is his work of any interest to the non-philosopher. Were he to enquire further, the general reader would soon discover that a grammaticist may be a Communist or a Tory, an atheist or a Roman Catholic, a militarist or a pacifist, a pillar of society or a Bohemian, without such attitudes either affecting or being affected by his work as a philosopher. He would rightly conclude that grammaticism has abandoned one of the traditional concerns of philosophy, the one of most interest to the general reader: it is no longer a criticism of, nor does it bear upon the criticism

of, opposing ways of life. Logical positivism might annoy the general reader, but it was obviously relevant to the criticism of political and religious doctrines; grammaticism is more likely to bore than to disturb.

This has been the reaction, certainly, of a great many professional philosophers. *The Concept of Mind*, it is true, has caused something of a stir, but mainly because it has been interpreted as a re-statement of the case for behaviourism, a point of view which obviously does have social and religious implications. For the most part, grammaticism has gone its own way, met by rumblings of discontent rather than by critical consideration.

Even those who feel that the task is worth undertaking—as I do myself—may well hesitate to criticise grammaticism in any but the most sweeping of fashions. The grammaticists are determined to avoid generalisation; they supply examples of the grammaticist method rather than descriptions of it. This means that the critic has either to play the grammaticist game by suggesting modifications at this point or that to the treatment of a particular example—which does not accomplish his more radical ends—or else, which will provoke much shaking of the head and pursing of the lips, he must himself attempt to generalise about the character and purpose of grammaticism. And this is by no means an easy task; often enough one appreciates only too well the feelings of Peer Gynt when he was confronted by the Great Boyg.

Of course, it is easy enough to see that the grammaticists are "always talking about language". But they are not unique in their close attention to linguistic points. Grammaticism is now sufficiently respectable to feel the need of ancestors, and Mr. Flew, in his editorial introduction, has no difficulty in showing that the British empiricists, too, "talk about language"—and, more than that, regularly accuse their opponents of being led into confusion by their verbal usages. Yet the British empiricists are not grammaticists. The British empiricists distinguish sharply between *nominal* and *real*

solutions of philosophical problems; their scholastic predecessors, they argue, pretend to solve philosophical problems by the simple device of inventing a terminology, instead of offering a genuine solution of them. In other words, the British empiricists attack the scholastics precisely in the spirit in which anyone might now criticise contemporary American sociology. They want to clear away the verbiage in order to get at the real problem. Grammaticism, in contrast, denies that there is a problem; and philosophers are misled, in their view, not so much by the charms of a philosophical terminology—Locke is as guilty as Whitehead—as by the seductions of syntax.

Just what this means can be discovered to some extent from the first essay in *Logic and Language*, Professor Gilbert Ryle's *Systematically Misleading Expressions*. Unfortunately, this is a relatively early essay (it first appeared in 1932); it has an historical interest in so far as it announces Professor Ryle's conversion to the grammatic cause, but there is a good deal in it which he would now wish to renounce, as the metaphysics of his unregenerate days. Nevertheless, it displays very clearly some of the strong points and certain of the weaknesses of grammaticism; for that reason, it will repay detailed consideration. And, much more than most grammaticist writings, it lays its cards on the table.

Professor Ryle begins with a pronouncement about the history of philosophy. "Philosophical arguments", he says, "have always largely, if not entirely, consisted in attempts to thrash out 'what it means to say so-and-so'" (p. 11). This is an extraordinary interpretation of the history of philosophy, but it seems to have been current at Oxford and Cambridge, and Ryle does not regard himself as being under any obligation to defend it. He sees, however, that it is only tenable on a rather special interpretation of "what it means". He points out that unless people are merely gabbling—in which case their utterances are of no interest to the philosopher—they already know what they "mean" when they use this or that

expression. "If an expression can be understood, then it is already known in that understanding what the expression means. So there is no darkness present and no illumination required or possible" (p. 12). If for "expressions" we substitute "concepts" the situation is not improved. Indeed, it is made worse, since "it is not true in any natural sense that 'there are concepts' ". And even if there are concepts, there can be no such thing as a *confused* concept. "Either a man is conceiving, i.e., knowing the nature of his subject-matter, or he is failing to do so. If he is succeeding no clarification is required or possible; and if he is failing he must find out more or think more about the subject-matter, the apprehension of the nature of which we call his 'concept'. But this will not be philosophizing about the concept, but exploring further the nature of the thing, and so will be economics, perhaps, or astronomy or history" (p. 13).

These arguments of Professor Ryle's are, I should say, in certain respects conclusive. But what do they show? In the first place, the philosopher is not concerned with what "it means to say so-and-so", in any philological interpretation of that phrase. If a person can use phrases like "honest men do not exist" in such a way that other members of his linguistic group can understand him, then he knows what (within that group) "exist" means, without the help of any philosopher. And otherwise it is for the school-teacher, not for the philosopher, to correct him. In the second place, a non-philological inquiry into "what so-and-so means" or "what a concept refers to" is always an enquiry into a certain subject-matter. To "clarify the concept of a planet", for example, is to engage in astronomy.

The philosopher, then, has not the general task of explaining "what it means to say 'so-and-so' ". But has any philosopher ever attempted, or imagined that he had to attempt, this task? Some recent writers have perhaps fallen into this absurdity. They have suggested that there is something called "analysis" or "clarification" which is neither a description of how words

are used by a linguistic group, nor the further exploration of a specific subject-matter. They have translated "what it means 'to be so-and-so' " (which might serve as a somewhat misleading description of a principal philosophical task) into "what it means to say 'so-and-so' " (which would convert the philosopher into a dictionary- or an encyclopædia-maker) as part of their general policy of identifying philosophy with linguistic analysis. Surely, however, most philosophers have supposed that it is their task to consider *certain* "concepts"—knowledge and goodness, for example—and would willingly admit that to examine these is in some sense at least, "to find out more or think more about" certain subjects. If Hume tells us "what it means to be 'a cause' ", he does so, partly at least, by persuading us to look more closely at the way events are connected with one another.

But, of course, Professor Ryle begins from the current presumption that if an enquiry is empirical, it must be no concern of philosophers. Thus, once having seen that "the classification of concepts" would be an empirical enquiry, he has to find another sense of "what it means" in which enquiry into "what it means" could be philosophical in character. He puts his conclusion thus: "There are many expressions which occur in non-philosophical discourse which, though they are perfectly clearly understood by those who use them and those who hear or read them, are nevertheless couched in grammatical or syntactical forms which are in a demonstrable way *improper* to the states of affairs which they record (or the alleged states of affairs which they profess to record). Such expressions can be reformulated, and for philosophy but *not* for non-philosophical discourse must be reformulated into expressions of which the syntactical form is proper to the facts recorded (or the alleged facts alleged to be recorded)" (p. 14).

Thus, for example, "carnivorous cows do not exist" looks as if it were about a certain kind of thing—a "carnivorous cow"—which is then said not to exist. This mode of expression,

although harmless in the mouth of the non-philosopher, may lead the philosopher to suppose that carnivorous cows must in some sense be "real", on the ground that it would otherwise be unintelligible to say that *they* do not exist. He may suggest, for example, that they "subsist" in a peculiar realm inhabited by all unrealised possibilities. Thus, in order to protect the philosopher, "carnivorous cows do not exist" ought to be reformulated in a less misleading form of words. (Professor Ryle suggests—after the *Sophist*!—"nothing is both a cow and carnivorous"). Similarly such assertions as "Mr. Pickwick is a fiction", "Virtue is its own reward", "Poincaré is not the king of France" may all mislead philosophers, in so far as they look like assertions of the form "This rose is red", whereas in fact their grammatical subject (in the case of "virtue") or their grammatical predicate (in the case of "a fiction" and "the king of France") is not, properly speaking, a subject-expression or a predicate-expression.

The procedure which Professor Ryle here employs, whatever one thinks about the details of this or that example, is certainly a valuable one. But the crucial question is how we are to show that a particular mode of expression is "improper" or "misleading", or that an alternative expression is "proper". The natural answer would be that the philosopher enquires into the structure of facts and then combines the results of this enquiry with his knowledge of the language, suggesting this or that mode of words as the least misleading way of conveying that structure. But obviously no grammaticist could give this answer.

What Professor Ryle suggests is that a misleading expression can be detected only by the fact that anyone who takes it literally will be led into paralogisms and antinomies. Consider, for example, "Mr. Pickwick is a fictitious person". Then the philosopher might be misled by the form of this expression into interpreting "fictitious" as a descriptive adjective, parallel to "benevolent", "comical", etc. But if he does, Professor Ryle argues, "Mr. Pickwick is a fictitious person" would imply

propositions such as "Mr. Pickwick was born in such-and-such a year", which "are not merely false but, on analysis, in contradiction with something in that from which they seemed to be logical consequences" (p. 35). The argument, presumably, is that since all persons are born in a certain year, it will follow, if Mr. Pickwick is a special kind of person, that he too must have been born in a certain year; and yet what we mean by being that kind of person ("fictitious") is, precisely, not being born.

Now, it is important to notice, what Professor Ryle's method of presentation conceals, that the premise "all persons are born in a certain year" is here presumed. No "antinomy" follows *directly* from "Mr. Pickwick is a fictitious person". A more accurate presentation of his argument would run thus: the propositions "a fictitious person is a person", "all persons are born in a certain year", "no fictitious persons are born in a certain year" cannot all be true. Then it becomes clear that the so-called "antinomy" can be avoided by anyone who cares to assert that only some persons (i.e., the non-fictitious persons) are born in a certain year, or that fictitious persons, as well as real ones, have a date of birth. And surely no one who is prepared to set up a world of fictitious persons would be so harsh as to deprive them of a birthday. The "antinomies" method hopes to avoid the direct confrontation of philosophical theories, the description of them as true or false. But it can only proceed by presuming the truth of ancillary propositions which might quite well be false; it cannot, that is, avoid direct assertion and direct denial.

Such an evasion, of course, is the purpose behind the grammaticist view that philosophical errors actually *arise out of* the misleading suggestions of language; we only need point out, it is presumed, that our language does not imply the theories which have been supposed to follow from it and then, at once, those theories collapse in ruins. "The detection of the sources in linguistic idioms of recurrent misconstructions and absurd theories" (p. 36)—that, according to Professor Ryle,

is the task philosophers must undertake. Now, perhaps philosophical confusions have sometimes arisen out of linguistic idioms; Meinong's views seem to be in the back of Professor Ryle's mind, and it may be that they had their source in an attempt to account for "the meaning" of assertions like "golden mountains do not exist". Even then, we may doubt whether the existence-subsistence antithesis would ever have occurred to Meinong had two-world theories not been already embedded in the philosophical tradition. And it is certainly implausible to suggest that such theories *in general* (which are at least as common in Eastern as in Western thought) are a response to the promptings of idiom.

Let us consider in more detail one case which Professor Ryle mentions—the theory of ideas. There are, as he points out, such idioms in our language as "the idea of having a holiday has just occurred to me". "And any way partly [can he be content to say "partly"?] through accepting the *prima facies* of such expressions", he continues, "philosophers have believed as devoutly in the existence of 'ideas' and 'conceptions' and 'thoughts' or 'judgments' as their predecessors did (from similar causes) in that of substantial forms" (p. 29). Now, one interesting fact about this example is that all these words—with the possible exception of "thoughts"—are of *philosophical origin*. "Conceptions" (in the form of "conceptus") was coined by a Latin commentator on Aristotle; and the Bishop of Worcester rebuked Locke for introducing that upstart technicality, the word "idea", into a book which professed to use "common words and expressions". The word "idea" is now part of common idiom, these facts suggest, only because the theory of ideas has gradually seeped down into everyday thinking: the theory originates the idiom, not the idiom the theory. At least, if the contrary is to be argued, it needs to be supported by detailed philological investigation.

In her essay on *The Philosophical Use of Analogy* (the fifth in *Logic and Language*) Dr. Margaret Macdonald supplies further examples of philosophical theories which "arise from

certain apparent peculiarities of ordinary statements". " 'This is red' and 'that is red', yet they are not two colours but 'the same colour' (problem of universals) ; 'we have both got tooth-ache but I can only feel my own' (problem of solipsism) ; 'That is a mirage though it looks exactly like an oasis' (problem of the existence of physical objects), etc." (p. 80). But once more, it is surely evident—except possibly in regard to the theory of universals—that these philosophical theories do not in the least arise out of the "peculiarities of ordinary statements" but rather out of a certain view about the nature of material objects, that Parmenides in Greek thought and Galileo at the beginning of the modern period gave birth to "the problem of knowledge" and that solipsism arises in the same context. The theory of universals is in a rather different position; in the *Parmenides* it is explicitly described as an attempt to point to the circumstances under which two things go under the same name; at the same time, it would be an error to suppose, even then, that this theory is "suggested by language". As is clear from the *Phaedo*, the theory arises within mathematics and ethics, as an attempt to describe the field of pure mathematics and the nature of a moral ideal; only then, and with some inconsistency, does it develop into a general theory of naming.

I draw attention to these historical facts with a great deal of hesitancy; I cannot help suspecting that the grammaticists must be quite well aware of them and do not, for some reason, regard them as decisive. Still, we need to remember that very many of the grammaticists were taught by men who were ignorant, and sometimes contemptuous, of the history of philosophy;² furthermore, it is part of the grammaticist policy to argue with contemporaries (preferably, even then, with students or with other members of the grammaticist group) rather than to discuss the work of their predecessors. Now, when a contemporary is asked why he

² Cf. Collingwood's attack in his *Autobiography* on the Oxford and Cambridge "realists". Waismann is the only writer in this volume who sees what his predecessors were about; he did not have a "realist" education.

believes that there are ideas, this question will probably astonish him, just as we shall astonish him if we enquire why he thinks it wrong to marry his sister. And if we press the point, he might draw attention (especially if he is the sort of person who has a great faith in "common-sense") to our ordinary linguistic usage. He might reply: "Everybody knows that we have ideas; a little while ago, for example, you yourself remarked that 'an idea has just occurred to me'". Similarly, if we press the question about marriage to his sister, the reply might come: "Inbreeding has bad biological results". Now, in this latter case, it is easy to recognise that opposition to marriage between brother and sister is not in fact a deduction from biological consequences; views about incest are part of the tradition of our society and will, in most cases, have been taken over without question from that tradition. And that, too, is why most people believe there are "ideas". If we want to enquire into the origin of these traditional beliefs we are driven beyond the questionnaire into history; if we are content to demolish the "reasons" with which our contemporaries, under pressure, supply us, we may win easy dialectical victories but shall be no nearer understanding why philosophers have thought as they did. Certainly, we shall not have explored the *origins* of their beliefs, as the grammaticists profess to do. That is one of the most puzzling features about grammaticism; it propounds what is clearly a historical thesis—that philosophical beliefs originate in a misunderstanding of idiom—and yet it seldom supports that thesis with historical evidence. One would expect arguments to show that cultures in which certain idioms are unknown are also quite free from the sort of philosophical theory to which those idioms are supposed to give rise; one would expect, also, detailed historical analyses directed toward showing that at this point or that the great philosophers have been led into error by the enticements of idiom. But we find no such argument. Are we to understand that the grammatic thesis is so obviously true that it needs no more support than the sketchiest references to "what philosophers say"?

It is true that Dr. Macdonald, in a manner quite unusual, quotes here and there from the classical philosophers. Some of the suggestions she makes are of considerable interest, and with her general thesis—that philosophers often depend upon misleading analogies—many of us would have no quarrel. At the same time, many of her criticisms are more than a little hasty. Discussing, for example, Descartes's view that "when I distinguish the wax from its exterior forms, and when, as if I had stripped it of its vestments, I consider it quite naked . . . it is certain that nothing remains except something extended, flexible and movable . . . of which the perception is neither an act of sight, of touch, nor of imagination . . . but is simply an intuition of the mind", she comments thus: "This is surely a very curious procedure. When its sensible qualities have been 'stripped' from a perceptual object nothing remains which could conceivably be perceived at all" (p. 86). Now, of course, if "perceived" means "sensorily perceived" Descartes would wholeheartedly agree; in this sense of "perceived" it is indeed tautological to say that only "sensible qualities" can be perceived. But the point he is trying to make is that these are not the qualities with which a scientific account—which he hopes to identify with a *geometrical* account—of the wax would concern itself; hence the wax as it "really is" is *not* sensorily perceived but is an "intuition of the mind". No one can hope to understand Descartes who insists on assuming that "perception" has, in a translation of Descartes, its modern sense. Grammaticists seem to approach philosophers (as writers like Prichard did) in search of an occasional passage which can be interpreted, out of its context, as supporting their thesis. But only by a really thorough historical treatment can we hope to understand how philosophical theories arise. Such a historical treatment, I should add, would have to pay close attention to the relations between science and philosophy, for the misunderstanding of science has been, and is, a far more important source of philosophical error than the misunderstanding of ordinary idiom.

Even in the present case, we can understand the origins of the grammaticist error only if we remember that philosophers have taken over from scientists the assumption that scientists have by now parcelled out the world between them, so that the philosopher has somehow to say something without having anything *in particular* to say. In a comical fashion, the grammaticist is forever insisting that he has nothing to tell us. "What the philosopher does", says Dr. Macdonald, "is to notice certain differences in our use of ordinary words which seem to require explanation by a theory about that to which the words are said to refer. But it may be that what is required is just a description of these different uses and their criteria. If this is to be called a 'theory', very well, but it must be noticed that this is a very different use of 'theory' from that employed by science and the C.I.D. or even in ordinary life when we contrast 'theory' and 'practice'." (p. 95).

But just how is it different? Let us suppose that the philosopher is concerned with describing "different uses and their criteria". How is he to proceed except by saying: "My hypothesis is that this use is different from that in respect of Y", and then submitting that hypothesis to various kinds of test? Some clue to what the grammaticist takes to be an alternative is supplied later, when Dr. Macdonald writes: "Nor is the statement that philosophical propositions are verbal to be taken as a piece of brand-new information about them as the composition of water was a piece of new information for eighteenth-century chemists. But it draws attention to the fact that *the criteria* for the truth or falsity of philosophical propositions are the uses of language" (p. 99, *my italics*). The suggestion seems to be that there is something called "the criteria" (not *my* criteria or *your* criteria but *the* criteria) for using words—in this case the word "philosophical"—and that the philosopher merely reads off the criteria, without having to discover them by any process of enquiry. He doesn't, then, put forward the view that words are used in this way or that *as a theory*; he merely *tells* us

that this is how they are used, and even this, it is presumed, is something we "really" know already. We have heard similar stories before now from dogmatists of one persuasion or another; nothing better can be expected once it is supposed that the philosopher proceeds by a method which is in no way scientific, which does not employ trial and error. But these dicta are unlikely to convince those who have not been vouchsafed the grammaticist revelation.

With these general considerations in mind, let us turn to later essays in the volume, and first to Professor J. N. Findlay's *Time, A Treatment of Some Puzzles*, which was originally published in this Journal (Vol. XIX, No. 3). Professor Findlay remarks that he is "now inclined to attach rather more positive value and importance to the metaphysical perplexities and positions" with which his article concerns itself than he was at the time at which he wrote it; we shall, however, take it at its face value, as a contribution to grammaticism, and one which discusses a topic peculiarly susceptible to grammatic analysis.

Professor Findlay begins by pointing out that we do not, in practice, find it difficult to cope with temporal situations; everyone knows when to say that such-and-such "happened yesterday" or "took a long time" or "will happen soon". Why then do people profess to find time strange? They must surely be suffering from a peculiar disorder and it is our business, Professor Findlay suggests, "to cure them of this disorder through a clear awareness of its causes" (p. 38). (This article exemplifies what has been called "therapeutic positivism".)

A person who "finds time strange" is not making a "mistake", in the ordinary sense of that word. If he were, we could free him from his error by pointing to facts which he has overlooked, whereas we know that "he *has* all the kinds of experience which could throw light on his problems, and yet he is puzzled" (p. 39). How is it possible to be deluded without having one's facts wrong? "We are brought back to

the supposition that there are some questions which beset us, not because there is anything genuinely problematic in our experience, but because the ways we speak of that experience are lacking in harmony or otherwise unsatisfactory. We are sometimes thrown into a mood of interrogation not because we are in quest of further facts, but because we are in quest of clearer, or less discordant, or merely different ways of verbally dealing with these facts. Such moods of questioning plainly have no 'answers', in any ordinary sense of 'answers'; we may nevertheless hope to relieve them by becoming clearly conscious of the underlying needs that prompt them, and by deliberately adopting ways of talking that provide appeasement of those needs" (p. 39).

Now, I should not wish to deny that when people are "puzzled by time" their puzzle is sometimes of a very peculiar sort. A good deal of philosophy is certainly an attempt, in Joyce's phrase, to "beat time"; philosophers, and a great many people who are not philosophers, find time unpleasant, distressing, incompatible with their hopes. It is sometimes difficult to explain what a person is doing who "doubts whether time really exists", except by comparing him to the singer who wants the Universe rolled back to give him yesterday. One can fairly enough point out, in Professor Findlay's manner, that his "doubt whether time really exists" doesn't prevent a person from complaining if we are late for an appointment; and may wonder precisely what *would* count as evidence for "the reality of time". At the same time, philosophers do not merely *assert* that "Time is unreal"; they produce arguments, raise difficulties, and unless these arguments are merely footling, it would be the height of sophistry to allege motives for their conclusions, or to set out to show "how their error arises", without any consideration of those arguments themselves.

These arguments very often take a form which is closely parallel to Professor Ryle's method of demonstrating that an expression is "misleading". Temporal assertions, such as

"A is later than B", are, it is argued, perfectly well understood by ordinary people, thinking as they do at a superficial level; and when we are speaking with the vulgar, we may make use of them ourselves without this giving rise to any misunderstanding. But the "is" in "is later than" may suggest, falsely, that Time is Real, as distinct from Appearing. But this cannot be, because to take Time in this way leads to antinomies and paralogisms. In reply, Professor Findlay argues that no contradiction arises except in so far as the ways in which we talk about time are "lacking in harmony"; and this is a difficulty we can easily overcome, if it really troubles us, by altering our linguistic habits.

Take, for example, the word "now". We say "the band is now playing *Pinafore*", and we are understood. But suppose we encounter a dialectician. *Pinafore*, he objects, is a *sequence* of notes; they cannot all be played "now". What *precisely* is the band now playing? We answer by referring to a certain chord; but a chord, the dialectician points out, takes a certain time to play—what part of it is the band playing *now*? No answer will satisfy our questioner. However fragmentary the passage to which we refer, it will still be a sequence and, therefore, the dialectician will tell us, cannot be played *now*. If we try to evade this conclusion by arguing that the band takes no time at all to play the passage we describe as being played now, so that it is not a sequence, we cannot explain, the dialectician will point out, how a series of such timeless notes could together make up the temporal sequence we call *Pinafore*.

Professor Findlay answers the dialectician with a "Fay ce que voudras". We can say what we like, he tells us, either that some events last for a certain time without being a sequence, or that some events take no time at all and yet can together be a sequence, or anything else which makes us feel happier. "It does not in fact matter, in all this choice of diction, *what* we say, provided only that we truly please ourselves: the facts are there, we can see and show them,

and it is for us to talk of them in ways which will neither puzzle nor embarrass us" (p. 46).

One must, I should say, tread carefully in any criticism of this "short way with dissenters". Consider a case in some ways parallel. Somebody sets out to "use the word 'large' consistently". All he can possibly do is to decide quite arbitrarily that he will not call any thing 'large' unless it has dimensions beyond such-and-such an area; and this, of course, will mean introducing a new use of the word "large", because that ordinarily means "larger than some unspecified although vaguely recognised size". Similarly, we know what is meant by "now" in such expressions as "it is spring in England now", "Mr. Jones can see you now", "now is the time to buy"; it refers us to a period of time which is not specified and which varies with context, but which nevertheless is as exact as we need it to be. To be "precise" we should have to decide, quite arbitrarily, that "now" refers, for example, to a period of half a minute after the word is uttered (presume, for the sake of argument, that this is an exact specification); and this would be introducing a new usage as distinct from describing an old one. But this is only a preliminary point; it scarcely touches the main argument. The impetus behind attempts to take "now" strictly does not come from any "misunderstanding of the logic of our language" but from atomism, as can be seen clearly enough in the philosophy of Hume. Professor Findlay is, I should say, mistaken in suggesting that difficulties about time, *as they arise in the writings of philosophers*, "form a relatively self-contained group of puzzles" (p. 39); on the contrary, they ordinarily arise out of the attempt to apply a general method of explanation, supposed to work quite effectively in other fields, to temporal situations. This, for example, is the importance of those arguments which set out to show that a temporal sequence is not a sequence of just so many non-sequential "instants"; they draw attention to the fact that

the methods of analysis characteristic of atomism will not work when applied to temporal situations.³

Professor Findlay's reply is that we can always make them work by changing our verbal habits. And in a way, of course, this is true. A person could, if he liked, say that "taking no time" means "taking less than a second", and could then go on to maintain that every temporal sequence is a series of sequences which "take no time". But nobody does so like, nobody is under the least temptation to talk in this way. Of course, a man who would be happy to say, as Professor Findlay allows him to do, that "mere nothing can at times have definite properties" will not need our permission to say what he likes. But the real question is whether people can say all the things they have actually wanted to say, using words in the way they have actually wanted to use them, without either falling into inconsistencies or denying what seem to be facts. The sort of consideration which Professor Findlay mentions as if it were merely incidental (as when he says, talking of the view that Zeno's paradoxes leave us within a world in which we are "desperately immobilized", that this condition, if it were anything, would be a state that "lasted and took time" (p. 50)) is, I should say, crucial. If philosophers are obliged to talk with the vulgar even in the very act of denying what the vulgar affirm, this is a clear indication that their views are untenable. And to say that they could, if they liked, avoid talking with the vulgar by using all their words in a different sense is to imagine that a rout can be turned into a victory by describing it as "a tactical withdrawal".

From the remaining essays, although they contain a great deal which it would be interesting to examine closely, I can comment (at the risk of misinterpreting) only on an occasional passage in order to illustrate further why some of us are dissatisfied with grammaticism. Mr. Paul Edwards, in his

³ Of course, I do not mean that they will in fact work in other situations. But time has often been the rock upon which philosophies have most obviously come to shipwreck.

Bertrand Russell's Doubts About Induction, is mainly concerned to criticise the account Russell gives of induction in his *Problems of Philosophy*. In the course of his argument, he comments as follows: "When Russell says that observed instances are never by themselves a reason for an inductive conclusion, he is guilty of an *ignoratio elenchi* by redefinition since we never in ordinary life use 'reason' in Russell's sense when we are talking about inductive arguments" (p. 68). One can only reply: "So what?" Russell is setting out to make a contribution to philosophy; it surely does not matter in the least how ordinary people use the words he is employing—the only question of importance is how they have been used by philosophers. Mr. Edwards, indeed, might rather be accused of an "*ignoratio elenchi* by redefinition" for using "reason" in its ordinary sense (in so far as it has an "ordinary sense") in a *philosophical* discussion. If it could be shown that Russell's argument is only plausible because he sometimes uses the word "reason" in one sense and sometimes in another, that, of course, would be a point of importance; but there is not the slightest reason why he should be content to use the word in its "ordinary sense".

In the end, indeed, we are left more than a little puzzled about Mr. Edwards' intentions. Is he telling us that people normally say that they "have a good reason" for believing that X is Y if the X's they have noticed have mostly been Y, and there are no counter-indications present? I should not have thought that Hume or Mill or Russell was ignorant of this fact. But they also knew that people say they "have a good reason" for believing that X is Y, when for example they "feel it in their bones" or "see it with their own eyes". We shall never understand induction theory unless we remember that writers like Mill and Russell—and, to a certain degree, Hume—are anxious to show that science is better than superstition, that certain sorts of what people ordinarily call "good reasons" (that sort which, so they thought, science provided) are better than other kinds—and "better", they

thought, means "more like proofs". Does Mr. Edwards wish to suggest that they should have been content simply to notice how the words "good reason" were used in ordinary life? He seems, indeed, to fluctuate between a descriptive and a prescriptive use of "good reason" precisely in the manner Mr. John Mackie has detected in Mr. Stephen Toulmin's *The Place of Reason in Ethics* (this Journal, August, 1951).

A further curious point is that Mr. Edwards never produces any evidence that such-and-such is in fact the ordinary usage. Once again, as in the case of Dr. Macdonald's "criteria", we are merely presented with *obiter dicta*. Take the following case: "When an ordinary person says that water of 50° C is hot, all he means is that human beings, with their body temperature being what it is, would in *all ordinary circumstances* have sense-impressions of heat on coming into contact with such water" (p. 63, original italics). How has Mr. Edwards extracted the information that this is "all an ordinary person means" when he says that water is hot? Obviously not by any sort of questionnaire; "sense-impressions" and "body temperature" do not even form part of the vocabulary of an "ordinary man". Indeed, if we were to ask an ordinary man what he means by "hot", he would, I imagine, look at us in great surprise, unless he supposed us to be foreigners. And he might quite well be astonished if we told him that "all he meant" was something about the water's effect on human beings, as distinct, for example, from its effect on frozen pipes. Just how he would react I do not in fact know; but does Mr. Edwards know either?

Mr. George Paul, in his *Is There a Problem About Sense-Data?*, has, like Professor Findlay, a topic to discuss which in some measure lends itself to grammaticist analysis. For certainly arguments about sense-data are beset to a prodigious extent by ambiguities and verbal confusions. It is worth pointing out, as Mr. Paul does, that the truth of propositions like "this appears to be so-and-so" does not

imply that there are "appearances"—unjustified transitions from verbs to abstract nouns have certainly been a feature of epistemological discussions. And Mr. Paul himself recognises that his article consists of prolegomena: "I do not deal with *the* question about sense-data, which gives point to the introduction of the word at all" (p. 116). What may be doubted, however, is whether, just because he leaves aside the central issues, Mr. Paul is able to deal with the questions which he regards as preliminary, except in a somewhat arbitrary manner. At crucial points, indeed, Mr. Paul seems simply to assume the truth of precisely the propositions which would be most hotly disputed. For example, he argues that in answer to the question: "What am I seeing when I see a penny which looks elliptical?" we can reply "It only seemed to me that there was a round penny which looked elliptical. I was really not seeing anything at all" (p. 108). Surely, what more than anything else lies behind the conviction that there are sense-data is the feeling that we *cannot* answer in this way; that we must have been seeing *something*, if we were "seeing" at all. And again, while he denies that "in order to give a complete and accurate account of any perceptual situation it is necessary to use a noun in the way in which 'sense-datum' is used" (p. 109), he certainly does not make his denial good; nor does he show, what he also suggests, that it is *possible* to use the sense-datum terminology (in the way in which it is in fact used) without falling into inconsistencies. And has anyone ever in fact supposed that "the use of the word 'sense-datum' [would] help to free us from any of the difficulties we get into about our use of words for physical objects"? It was not "the use of the word" but the supposition that there were entities would could be "picked out" (in Moore's phrase) as "sense-data" which was supposed to help us. If, as Mr. Paul's argument does something to show, *there are no such entities*, the sense-data theory simply collapses. And again it was not "our use of words for physical objects" which philosophers found difficult, but rather the

fact that many people wanted to say simultaneously that physical objects really have certain qualities, that we perceive physical objects as they really are, and that we do not perceive them (usually) as having these qualities.

Mr. H. L. A. Hart's essay on *The Ascription of Responsibility and Rights* is one of the most interesting in the volume. He sets out to consider the force of assertions such as "He did it". These, he says, are usually regarded as descriptive, but they are in fact ascriptive; they ascribe responsibility for actions. "He did it" means "he was responsible for doing it". It is commonly supposed that "he is responsible for doing X" is *itself* descriptive, meaning something like "he exerted an act of will before he did X". But this, Mr. Hart suggests, is an error; he illustrates the nature of the error by referring to comparable cases in the law of contract. We might imagine that there must be a true proposition of the form "an act is a contract unless . . .", where some simple limitation follows the "unless". But in fact, although legal theorists have often tried to lay down such a simple limitation, there is none to be found; the various conditions under which we can plead "no contract" cannot be summed up in a single phrase. Similarly, there is a variety of circumstances under which we might say "He didn't do that"—he acted in self-defence, he was mad, it was an accident, etc.—and there is no psychological event common to these various circumstances.

This sort of analysis can be extremely valuable, whatever we may think of the details in this case (would not Mr. Hart's view make "he did that accidentally" nonsense, and "he did that deliberately" tautological?), or however we may wonder why Mr. Hart is so confident about what "we" would say. It is certainly important to recognise that what look like descriptions may actually be ascriptions. And Mr. Hart's use of legal theory revives the old tradition that the special sciences can both illuminate and be illuminated by the work of philosophers. Those philosophers who dismiss the grammaticist movement *en bloc*, this essay serves to remind us,

fail to realise that valuable work can be carried out in its shadow.

Finally, Dr. Margaret Macdonald's *The Language of Political Theory* illustrates what we may hope will be a developing tendency within grammaticism. Not unnaturally struck by the mythological character of the classical political philosophers (with their "social contract", their "natural law", their "real will" and the rest) she is still not content to dismiss them as nonsensical. She attempts to consider, rather, what social facts, and what political aspirations, lead men to construct, and to cling to, theories of so odd a kind. But as soon as Dr. Macdonald attempts this task, her argument reveals with special clarity the curious presumption which, as I have already argued, runs through grammaticist writings, that there is something describable as "we", who operate with "criteria". Thus, taking as her examples "why should I obey the Conscription Act?" and "why should I oppose the present German Government?", she says "we all know the kind of criteria according to which we should decide these two issues" (p. 184). One can only presume that "we" means "Dr. Macdonald and her friends". Does she imagine, for example, that a supporter of the Nazis would use the same "criteria" as an English liberal? This is not an isolated case; in reading the grammaticists one is constantly struck by their inability to imagine that anyone might be guided by principles which are not *de rigueur* in contemporary Oxford and Cambridge circles—an "outsider" reads them with the same astonishment with which he contemplates the ethical writings of Sir David Ross.

I have described this article, in its title, as *Reflections*; it is not, and does not pretend to be, an exhaustive examination of the grammaticist movement. As I suggested earlier, there are special reasons why such an examination is as yet difficult to the point of impossibility. I should not be surprised to find that I have seriously misunderstood grammaticism; if I have, I do not think that my misunderstandings are unique. And

if communications in philosophy are to be maintained, it is as well that common misunderstandings (if such they should turn out to be) should be brought out into the open.

I hope I have made it clear that I am not one of those who think that the grammaticists have nothing whatsoever to say. Philosophy—and particularly epistemology, on which the grammaticists have laid special stress—is certainly rich in ambiguities and in theories which stand revealed as tautologies as soon as verbal distinctions are carefully made. Grammaticism has been useful in so far as it has emphasised that fact. But its criticisms have been incomplete. One cannot by a mere consideration of language itself show that a word is ambiguous or an idiom misleading. To pretend otherwise is to presume that it is already quite clear what distinctions are needed and to what truths we ought to be led. Grammaticism, I have suggested, takes for granted a uniformity of usage, of moral principles, of political beliefs, which has no foundation in fact. More generally, it presumes that all reasonable men are of the one mind. And this presumption looks plausible to grammaticists only because they approach all questions from a quite unhistorical point of view, as if only now, and in certain quarters in England, had common-sense, in the manner of Hegel's "Spirit", come to see itself as it really is.

THE NUMBER OF LOGICAL FORMS

By JONATHAN BENNETT.

1. Professor John Anderson's article (this Journal, May, 1952) entitled "Hypotheticals" has a wider scope than the title suggests, since it is concerned to show not only that hypotheticals are really A, E, I or O propositions in disguise, but also that there are no kinds of proposition but these four. Sections 7 to 10 of the present note will deal with Professor Anderson's treatments of hypotheticals in so far as they express a relation between terms or a relation between propositions. Sections 2 to 6 will be concerned with more general considerations relating to Professor Anderson's main theme that A, E, I and O propositions are all the propositions there are.

2. Before proceeding any further, it is necessary to look more carefully than Professor Anderson seems to have done at the meaning of the statement that such and such is not a logical form. Apparently Professor Anderson regards his case as more or less established when he can show that hypotheticals and the rest can be stated in A, E, I and O forms. But it must be recognised that this is a demonstration not that these others are not sorts of propositions, but only that they are not irreducible sorts of propositions. And from this another point follows: while "reducibility" is a necessary condition of Professor Anderson's making his case, it is not a sufficient condition. For reducibility works both ways, and were it the sole criterion we should have no trouble in eliminating all of Professor Anderson's four forms (*e.g.*, by re-stating them in terms of Russellian implication). What other criteria are there? "Economy" won't do: Occam's Razor may be good advice, but it is not admissible as a premiss in a philosophical argument. We seem, then, to be forced back to some such

claim as that A, E, I and O propositions are in some sense more basic than the other forms, or that they mirror more accurately the facts which they describe. It is by no means clear just how a proposition can be *like* a fact, but this criterion can be applied in a vague fashion in some cases, and its application will not support Professor Anderson's thesis. At this stage, however, the important point to note is that Professor Anderson has not succeeded until he has shown (a) that all other forms are reducible to his four, and (b) that the latter are in some sense most worthy to bear the burden. Most of what follows will be devoted to (a), but we shall glance at (b) to begin with.

3. In support of conclusion (b), Professor Anderson points out that his four forms are adequate to settle "the issue, 'Is it so or not?' ". Even granting (for the sake of the argument) that this is true, we may still ask whether this is the only sort of issue; whether, for instance, there are not also issues such as "Does it follow?", "Could it be so?", and "Are there any such?". If "Is it so?" is not the only sort of issue, then the fact that A, E, I and O propositions can settle all issues of this sort is irrelevant; and Professor Anderson gives no reason, apart from that implicit in his reductions to the four forms, for believing that this is the only sort of issue. It is fairly clear that at the back of his mind there is the thought that this must be the only sort of issue since it is the only sort which can be settled by the four forms—but this is a private motive which cannot be elevated to the status of an argument without circularity. This circularity is particularly evident in Professor Anderson's handling of existential propositions. On the assumption that the question "Is anything at all X?" *must* be of the form "Is any X Y?", it is not difficult to show that "existent" is a queer sort of Y for X to be or not be. But since this conclusion is used to justify the initial assumption, the demonstration is entirely inconclusive.

4. In any case, it is open to question whether every issue of this sort can be settled by one or other of the four forms. Professor Anderson makes considerable use of singular sentences such as might answer the question "Is it so?", but he gives no detailed reduction of the singular form, beyond the hint contained in the statement that what can locate can describe. This is relevant only if all location is *by* description, a view which is doubtful, as it seems to come to grief over statements like "I am a New Zealander". But if we ignore these doubts and follow up the hint, we presumably get a reduction of singulars to A's such that a statement like "Australia is the natural habitat of the kangaroo" becomes "All continental islands less than 1500 miles north-west of New Zealand are lands which are the natural habitat of the kangaroo", or something of the sort. The principal objection to this is that the singular asserts the singularity of its subject whereas the A doesn't—a logical fact about the meaning of the A which is not, of course, affected by the geographical fact that the A's subject class has only one member.

It has been pointed out to me¹ that singular statements like "Australia is *the* natural habitat of the kangaroo" must be formulated in a pair of A propositions, one the simple converse of the other. This certainly overcomes the difficulty that the singular is simply convertible whereas my suggested reduction is not, but it leaves the question of singularity untouched; identity of individuals is not the same as co-extension of classes.²

5. Professor Anderson gives no reason for rejecting conjunctive and disjunctive propositions and, indeed, he appears

¹ By Mr. John Mackie, to whom I am indebted for a number of comments on an earlier draft of the present note.

² Mr. Mackie says that Professor Anderson would deny this: "The single thing, Australia, is also a plurality of instances". I do not understand this statement, nor do I understand a reference of Mr. Mackie's to "all Australias". But even if there is anything in this treatment of singulars, Mr. Mackie, at any rate, concedes that "there is something peculiar about this plurality as opposed to other pluralities like 'All swans'".

to believe that they are equivalent to hypotheticals and their contradictories respectively. This would not matter if either conjunctive or disjunctive propositions could be reduced to one of the four forms in the same way as (Professor Anderson believes) conditionals can, but I know of no reason for believing such a reduction to be possible. And Professor Anderson's various reductions of hypotheticals are unfortunately not sufficiently clear (to me, at any rate) for the corresponding reductions of conjunctives and disjunctives to follow automatically.

Professor Anderson treats the proposition "X is identical with Y" as being equivalent to a pair of A propositions, XaY , YaX , which certainly suggests that he is allowing a place in his system for the conjunctive proposition $XaY. YaX$. And if it be replied that this is not a conjunctive proposition but merely a pair of A propositions, then we still have to explain the "togetherness" of the two propositions in the single identity. For, to deny that there is some sense in which the identity forms a unity would be to commit oneself to talking about "the *facts* that X is identical with Y", a form of speech which has about it the somewhat desperate air of a theory driven into a corner. Professor Anderson defends his views on the basis of the ways in which something can be the case, and he must surely admit that there are ways of being the case which cannot be expressed in a single A, E, I or O proposition, and which therefore require the use of conjunctive or disjunctive propositions. Nor can this conclusion be escaped by Professor Anderson's use of conjunctive *terms*, as the latter account only for those compound propositions in which the constituent propositions have either the same subject or the same predicate. This would not suffice, for instance, for the proposition "X is identical with Y", nor for its contradictory, "X is not identical with Y", which I take it Professor Anderson would reduce to "Either XoY or YoX ".

6. In any case his use of complex terms itself constitutes an admission which is fatal to Professor Anderson's case. For

the recognition of differences in the logical structures of various terms involves a recognition of different logical forms, *i.e.*, of different kinds of proposition. A logical form (Professor Anderson does not define the phrase) is simply the set of characteristics of a proposition in virtue of which other propositions can be inferred from it.³ And since propositions can be inferred from $XaYZ$ not only in virtue of its being an A but also in virtue of its having the kind of predicate which it has (for example, XaY and XaZ follow from it for this reason), we must recognise two forms of A proposition: those with predicates of the form Z and those with predicates of the form YZ . This applies *mutatis mutandis* to E , I and O propositions also, and thus at least doubles Professor Anderson's original number of ultimate types of proposition. (In fact, of course, it points the way to an infinite multiplication of the number of forms, *e.g.*, by the use of complex subjects as well as predicates.) The possibility of such a subdivision of each of the four forms puts Professor Anderson's thesis on the same level as would be a zoologist's attempt to show that there are only two zoological kinds — vertebrate and invertebrate.

Similar arguments, of course, can be given for other subdivisions of the four forms, *e.g.*, into those in which the terms are relative, those with secondary quantification, and so on. No doubt "Some foods are detested by all children" is a case of XiY , and to account for the validity of some of the inferences in which it appears we need say no more about it than that; but the secondary quantification must be treated as part of its form when we infer from it "Some foods are detested by some children" or "Some foods are detested by all English children".

7. In his attempt to show that the hypothetical as expressing a relation between terms is really just one of his four forms, Professor Anderson meets a difficulty created for him

³ Cf. C. H. Whiteley: "The Idea of Logical Form", *Mind*, Vol. LX, pp. 539-541.

by his general logical position. For if what can describe can locate, then $XZaY$ must be meaningless when XeZ , while what would seem to be the corresponding hypothetical, "If any X is Z , it is Y ", is meaningful. Professor Anderson says indeed that it is because the hypothetical is meaningful even when XeZ , and because we have confused it with $XZaY$, that we tend to believe that the latter too has meaning even when XeZ .

Now, it is clear that this application of the therapeutical method does not itself justify the view that under the given conditions $XZaY$ is meaningless: there is a difference between showing that a doctrine is true and showing why people believe it to be false. But the latter demonstration might throw some light on the former, and in any case if we are to accept (on any grounds) Professor Anderson's views on these two sentences, it is essential that we have some account of "If any X is Z , it is Y " which will explain its meaningfulness and the meaninglessness of what appears to be its "true" form, $XZaY$. It is therefore unfortunate that Professor Anderson's account of the matter, besides omitting to carry out the all-important reduction of "If any X is Z , it is Y "⁴ to the categorical form to which it is equivalent, contains a mistake which vitiates it entirely.

The conditional proposition "If any X is Z , it is Y ", says Professor Anderson, "besides conveying ZaY in the same way as if 'anything' were substituted for 'any X ', also suggests *arguments* containing the three 'existing' terms X , Y and Z (e.g., XaZ , ZaY , therefore XaY), and thus made up of intelligible propositions, whether they are true or not". Now, this just is not true. The proposition "If any politician is a logician, he is doomed to failure" does not "convey" the proposition "All logicians are doomed to failure", nor does it "suggest" the argument "All politicians are logicians, and all logicians are doomed to failure, therefore all politicians are

⁴ Mr. Mackie says that Professor Anderson *has* reduced it, in the two ways outlined in the quotation at the beginning of my following paragraph. Whether this is true depends, I suppose, on what Professor Anderson means by "conveys" and "suggests". I cannot be sure of what he intends.

doomed to failure". It does perhaps suggest the argument "All persons with the wrong sort of mental equipment are doomed to failure, and all politicians who are logicians are persons with the wrong sort of mental equipment, therefore all politicians who are logicians are doomed to failure", but this offers no support to Professor Anderson's distinction between $XZaY$ and "If any X is Z , it is Y ".

8. Professor Anderson's treatment of hypotheticals as expressing the relation of implication between propositions seems to depend largely upon his treatment of enthymemes. One gathers that Professor Anderson believes that "If . . . then . . ." propositions of this sort either express the validity of immediate or syllogistic inferences, or else express the validity of enthymemes which are just syllogisms in disguise. I shall deal later with his account of hypotheticals which express such facts as that XaY implies YiX ; first, there are some things to be said about implication-statements in general.⁵ The most important of these is that an enthymeme does not nominate its suppressed premiss. The suppressed premiss, for instance, of "Socrates is a man, therefore Socrates is mortal" might be "All men are mortal", but it might equally well be "All men who are sinners are mortal and Socrates is a sinner", or any of an infinite number of possible propositions. Thus, if it is the case (as Professor Anderson argues in his section B) that "If . . . then . . ." propositions often express the validity of an enthymeme, we must be clear that in such cases what they do is to assert that there is *some* proposition which, together with the antecedent, implies the consequent. (They do *not* assert *what* proposition it is which does this.) Now, if a proposition of the form "If p , then q " is interpreted as saying "There is some proposition r such that pr implies q ", then it still follows from this that there is *some*

⁵ The rest of this paragraph is due almost entirely to Mr. Arthur N. Prior. I have discussed Professor Anderson's article in detail with Mr. Prior and am indebted to him, in addition to this passage, for a number of other points made in this note, as well as for help in the arrangement of the material.

proposition r such that $r\bar{q}$ implies $\bar{p}$ (where r and q are the contradictories of $\bar{r}$ and $\bar{q}$), i.e., from "If p , then q " in this sense we may infer "If $\bar{q}$, then $\bar{p}$ " in the same sense, an elementary fact which Lukasiewicz would express in the symbols $CCpqCNqNp$. Similarly, from "If p , then q " and "If q , then r " we may infer "If p , then r "; i.e., any proposition or set of propositions which would complete both the enthymemes " p , therefore q " and " q , therefore r " would complete the enthymeme " p , therefore r ". Professor Anderson will surely be the first to grant that the meaning (and therefore the logic) of the "If . . . then . . ." statement does not vary according to the grounds on which it is asserted. And this means that he must allow a place for the set of logical principles (the so-called theory of deduction) dealing with the inter-relations of "If . . . then . . ." propositions, since it is only by the idealists' error of confusing evidence with meaning that we can be justified in refusing to "place" a hypothetical, trace its consequences, and so on, until we know why it is asserted—i.e., whether there is a suppressed premiss, if so what it is, etc.

9. But of course Professor Anderson is prevented from developing a great deal of this part of logic by his doctrine that false propositions have no consequences. Quite apart from its cramping effects, this doctrine seems to me to be quite certainly false. For though (as Professor Anderson points out) we can represent the process of falsification as the implication of the (true) contradictory of the original proposition by other (true) propositions, the fact remains that antecedent to any knowledge of a proposition's truth or falsity we know what propositions *would* falsify it (we know where to look for the evidence), which is tantamount to saying that we know what its consequences are. This process of falsification, of course, depends upon the so-called principle of antilogism, "If p and q imply r , then p and $\bar{r}$ imply $\bar{q}$ ", a self-evidently true proposition which, because at least two of its member propositions must always be false, Professor

Anderson is committed to denying. Furthermore, a falsifying argument in accordance with this principle would be of the form: "If p and q were both true, r would be true; but r isn't true, though p is; therefore q is not true". Professor Anderson reduces this simply to the argument " p and $\bar{r}$, therefore $\bar{q}$ ". This, however, omits a vital step in the argument, namely, " $\bar{q}$, therefore q is false". Professor Anderson's logic, in which false propositions are neither used nor mentioned, cannot say, for instance, that XaY and XoY cannot be true together, and thus cannot complete the last step in the falsification argument. And this is but one of the many ways in which this logic cripples any attempt to express the facts about implication.

In passing, two further points may be noted about the view that false propositions have no consequences. Firstly, it directly contradicts Professor Anderson's statement that "to arrive at a true consequence of p is not to prove p true". For Professor Anderson is committed to saying that, since only true propositions have consequences, to arrive at *any* consequence of p is to prove p true. Secondly, it may be remarked that this doctrine is just one manifestation of Professor Anderson's tendency to subject logic to non-logical considerations, a tendency which appears at several other points in his article: it lurks in the background when Professor Anderson seems to suggest that we can't know what an "If . . . then . . ." statement means until we know why it is asserted, and it comes into full daylight in the doctrine that we can't know whether $XZaY$ means anything until we have discovered whether or not any X is Z . If there is to be logic, it must be independent of the laboratory.

10. Apart from these difficulties, however, Professor Anderson must deal with the problem of hypotheticals which express the relation of implication between single propositions. Since he refuses to admit that statements like "If all swans are white, then some white things are swans" exhibit a form which is distinct from the A, E, I and O forms, Professor

Anderson must either deny them propositional status entirely (perhaps calling them "inference-tickets") or else claim that they are reducible to one or other of his four forms. One presumes that Professor Anderson would choose the latter alternative, though he does not carry out the required reduction. There is a hint as to how it might be achieved, however, in the suggestion that the major premiss of a hypothetical syllogism would be properly expressed in the form "Any A proposition implies the I with terms in the opposite order". If we follow this up we achieve a reduction of "XaY implies YiX" to some such form as "All propositions of the form XaY are propositions which imply a proposition of the form YiX"—though to cope with the proviso that X and Y are to be understood to stand for the same term in each occurrence, it seems to be necessary to subject this to a little revision, perhaps to something like "All pairs of terms such that the second is predicable universally and affirmatively of the first are pairs of terms such that the first is predicable particularly and affirmatively of the second". And though this is an A proposition, that is plainly not the whole story about its form. It is, for one thing, a proposition with relative terms, and, for another, a proposition in which the "relations" involved are themselves formal—"universal and affirmative predication", and so on. It involves, as it were, forms within forms. (Professor Anderson does not discuss the status of relations in his system, presumably because he regards it as obvious that any relational proposition can be reduced to a subject-predicate one. This is obvious, but, as I have already pointed out, such a reduction is not justified by the mere fact that it is possible. If, on the other hand, the reduction of relational propositions is justified on the grounds that subject-predicate propositions always "represent the facts" more accurately than their relational equivalents, then one can only reply that what is gratuitously asserted may be gratuitously denied. It is *not* the case that the proposition "Mr. Smith supports positivism by giving £100 for the purchase of philosophy books by

the library of Melbourne University" is a bad way of describing a fact which would be more accurately described by a proposition which "located" Mr. Smith and "described" him as "one-who-supports-positivism-by-giving" . . . etc.)

11. To sum up: Professor Anderson has not shown that A, E, I and O are the only *sui generis* kinds of propositions. In particular, he has given no method for avoiding the admission of singulars and conjunctives (or disjunctives), he has failed to give an adequate account of hypotheticals, and he has given no reason for thinking that every proposition must be of the subject-predicate form. Furthermore, he has failed even in respect of those propositions which can be reduced to the four forms, since these four are themselves subdivisible into many distinct kinds.

12. Finally, this whole approach to the problem of eclecticism is wrong in conception. To limit the subject matter of logic by recognising only the irreducible forms, besides leaving us with the problem of choosing between alternative sets of irreducibles, would be like "simplifying" mathematics by refusing to admit any constants but Peano's three indefinables. In each field an infinite number of "forms" (some reducible to others, some not) can be found. We don't study them all, but we do study those which are most useful and those which interest us most, and it is quite conceivable (though unlikely) that in so limiting our range of activity we should even ignore some of the irreducible forms. And since usefulness and interest depend to some extent on language, and to a greater extent on the complexity of thought of which we are capable, there is probably a theoretical limit to how far logicians can go—or rather, to how far they will bother to go. But there is no limit to logic itself; so I suppose we must say that, on Professor Anderson's understanding of the word, there isn't logic.

A NOTE ON "RELATIVISM".

By D. C. STOVE.

RECENT discussions in this Journal, between Professors Ryle and Smart on the one hand and Mr. Mackie and Mr. Foulkes on the other, have drawn attention to the central part apparently played in Professor Anderson's philosophic work by the quality-relation disjunction. This note will not attempt to expound or defend Anderson's views, but will try to show that disjunction is a special and misleading form of the principle on which Realism is based; and to suggest what the formulation of and evidence for that principle are.

The source from which the disjunction, historically even if not logically, flows, is the rejection by Anderson and other Realists of any type of Idealism in theory of knowledge. The basic logical error of Idealism, which he calls "relativism", has been found by Anderson to occur in many other philosophic theories as well. In most of these other cases, especially in ethics and psychology, qualities have been prominent, though in the original case (Idealism), significantly enough, they were not, since obviously "being the object of knowledge" is not itself a quality, and a given object might or might not be a quality.

What is "relativism"? I try to answer this by considering the principle which is held to refute it.

There is in Anderson's writings a number of different formulations of this principle, but I think its force is best seen if we take it as "A term cannot be constituted by its relations". Given this, together with the additional premiss that knowing is a relation, Anderson can demolish Berkeleian "sensible things", Cartesian "thinking things"; and again with the additional premiss that commanding or requiring

is a relation, he can demolish moralistic ethics; and so on. Now certainly if the above principle can be maintained, we can safely substitute in it the word "quality" for the word "term". Perhaps that is even an especially important substitution to make. But we do not *have* to make it, and my main point is that we can equally well substitute "relation" for "term" in the original: "a relation cannot be constituted by its relations". And the evidence for this will be precisely what it was for the principle when it was asserted of qualities.

Further, if we substitute in the original the word for some type of term (category?) other than either quality or relation, the evidence for the principle is still unchanged: "a quantity cannot be constituted by its relations"; "a number cannot be constituted by its relations".

What is the evidence? What is wrong with the contradictory of any of these "principles"? Simply that if there is a relation, one of the terms of which is "constituted by" the relation, then the relation is one term short. (And if more than one term is so "constituted", we are more than one short.) But the principle is certainly indifferent with respect to the type of the terms: they might be qualities, relations, quantities, or what have you. The error of "relativism" can now be seen as a direct contradiction: it is asserted that XRY , which has sense only if Y is *not* $\bar{R}$, and is not $\bar{R}X$ ($\bar{R}$ = the converse of R); but also it is asserted that Y is $\bar{R}$ (e.g., Y 's *esse* is *percipi*), or that Y is $\bar{R}X$ (e.g., Y 's *esse* is *percipi* by an active being).

Another way of formulating the point would be to say that the refutation of relativism lies in or is "proclaimed by" the meaning of "relation" or of any particular relational word. Then a thorough-going relativist would be a thinker who wanted finally to drop "relation" and all relational words. That is why the Absolute Idealists, *qua* monists, are the most thorough-going relativists.

I think this view of the matter will be supported by reference to Anderson's most extended remarks on the topic, in the first part of "Realism and Some of its Critics" (this

Journal, Vol. VIII, No. 2). Certainly the brief reference to qualities there seems inessential. But if I am right then at least two things follow.

First, it follows that the proposition "No qualities are relations" is not established by the refutation of relativism, but independently if at all. It may be possible to furnish such an independent argument, but the difficulty of giving anything like a differentia of "quality" makes me suspect that this proposition is only a rough and misleading form of the general principle.

Secondly, it follows that when any empirical concept is under discussion, it is illegitimate to come forward armed only with the allegedly exhaustive pair of possibilities, quality and relation. Or rather, the refutation of relativism does not legitimise this (as it is thought to do). The empirical application of this apparatus seems to me to lead either to the doing of violence, as in the case of Anderson on mind, or to the provision of decent accommodation for entities which deserve only total eviction, as in the case of Foulkes on force.

So that the principle proves neither that nothing is both a quality and a relation nor that everything is either a quality or a relation.

Relativism is not false but illogical, and therefore, however the principle of its refutation is properly to be formulated, it is not a piece of empirical information. I suggest the following further analysis. Taking the principle in the form of "a term cannot be constituted by its relations", its truth follows analytically from the occurrence in it of the word "its" (cf. Anderson, *loc cit.*, p. 118) in conjunction with any phrase playing the role that "constituted by" does in it. Any such phrase will mean, as the latter does, "identical with", and this reduces the principle that a quality cannot be constituted by its relations to precise parity with a principle that a quality cannot be constituted by its qualities, either. The general principle would thus be reduced to "a term cannot

be constituted by its . . .", where the sentence can be completed by the word or phrase for any type of term whatever. It would then amount to saying that a term cannot be identical with any other term(s), and one could not very well regard this as synthetic. Nevertheless this may well be the real force of the principle, because the "independence" of terms with respect to their relations, in particular that of knowledge, which Realism asserts, is not of an empirical character (Realism is not a theory of perception), but is purely logical: the independence of whatever is logically diverse. (Cf. Hume on the "substantial" character of *any* perception.) Correspondingly, the Idealist denial of independence to the "object of knowledge" is not empirical either, but is the denial of the ("ultimate") diversity of knower and known; if we take Berkeley at his word; his dictum becomes tautologous. But however prominent the treatment of "relations as forms of identity" (Anderson, loc. cit., p. 117) has been, *e.g.*, in Idealism and in pre-Humean causality, the fact remains that XRY (where "R" is any particular relation) and X is Y ("is" of identity) are incompatible.

This suggested reduction of (in effect) Realism to what may be taken as the principle of empiricism would explain something that needs explaining, *viz.*, why it is that whatever is criticisable as relativistic is always alternatively criticisable as involving necessary connexion. A term whose "nature" it is to have a certain relation, or which is "internally" related to another term, is for an empiricist not really being distinguished from the relation, or rather is being both distinguished from and identified with it.

CRITICAL NOTICE

PLATO'S THEORY OF IDEAS. By Sir David Ross. (Oxford, Clarendon Press, 1951. 251 pp.) Price (Aust.), 30s.

After laying down the approximate order of the composition of the dialogues (determined mainly by stylometry), Sir David Ross traces the theory of Ideas (or Forms) through the dialogues in this order, and then gives an account of Plato's latest views. Some of his most interesting discussions are those of the passages in Aristotle which throw light on such topics as the Ideal Numbers and "the population of the world of Ideas". Ross assumes that the thought that is expressed in the dialogues is that of *Plato* throughout: he completely rejects the Burnet-Taylor theory, taking it to have been already refuted by other scholars and by himself in other writings. Though I cannot argue the points here, I think that this rejection is too complete, and also that in some of the later dialogues Plato at least hints at a new theory, distinct from the theory of Forms.

The value of this book cannot be denied; Ross brings together in a remarkable way the evidence of the Greek texts and the arguments of modern scholars on each problem of interpretation as it arises, and while his own conclusion at each point is clearly stated the rival views are given a very fair hearing.

Of course, a great many detailed questions could be discussed further, but the main points I wish to take up are two very general ones, closely connected with each other, about the precise nature and origin of the theory of Forms.

In his summing up (p. 225), and in many places throughout the book, Ross takes the theory of Forms to be essentially the assertion that there are universals, "a class of entities . . . entirely different from sensible things"—and he believes

that this assertion is true. Whether the Forms are immanent in sensible things, or transcendent, is a further and quite minor question, to which Plato gives no single clear answer. Ross repeatedly suggests that the consistent view which Plato *approaches* is that there is a Form for every "common name". Correspondingly, he finds the origin of the theory in the question how a number of things may all be called by the same common name—this question arising out of Socrates' search for definitions of temperance, courage, and so on.

Now I suggest that this is a one-sided account, that while these elements are present more emphasis should be given to other aspects of the theory and to other influences that helped to produce it.

In addition to their role as universals, the Forms have other roles to play. Ross recognises their importance as the field of eternal truths, which are "relations between universals", but he underemphasises their role as ideals and standards. And whereas he says that his object is "to trace the history of Plato's views rather than to assess their value" (p. 165) it seems that an adequate account would have to bring out the difficulties in those views, in particular the tension between these various roles—the very point at which Aristotle is hinting in the passage (*Mét.* 990b8-17) that Ross has quoted on p. 165: not everything that receives a common name, nor every object of science, is an ideal. Ross's arguments in Chapter XI do not really show that Plato regularly took a very broad view of "the population of the world of Ideas". Ross makes it quite plain elsewhere that it is the moral and mathematical Forms of whose existence Plato was most fully convinced. And there seem to be at least two instances of a limit to this population. (i) Plato did not recognise an Idea of number in general (pp. 181-2)—yet "number" is a common name. (ii) There is a serious problem whether Plato recognised Forms of artefacts, such as Bed (pp. 171-5). They are mentioned in several dialogues, but Aristotle (apparently supported by a fragment from Xeno-

crates) says that Plato did not admit them. After discussing several unsatisfactory solutions of this problem, Ross accepts that of Robin (the rather makeshift one that Plato rejected only "Forms of the products of the imitative arts"), but he does not mention the most likely solution, that Plato was drawn in different directions by the different aspects of his theory. "Bed" is a common name, and there is also an ideal or standard to which the craftsman looks, but (in spite of what Ross says on pp. 174-5) it is hardly a subject of eternal truths, hardly something which God had to take as given when He made the sensible world. Along these lines we can understand an admitted inconsistency, that in the *Republic* Socrates speaks of God as the maker of the Form of Bed, while in the *Timaeus* and elsewhere the Forms are independent of any maker; for once you admit Forms of what are essentially artefacts you are practically compelled to introduce an artificer for them, if you are also treating the Forms as "perfect particulars".¹

This is, indeed, another role that the Forms normally play, though in the end it seems to have been taken from them and assigned to the "mathematicals". It is incompatible with their role as universals, but it obviously controlled a great deal of Plato's thought, as well as his language, and cannot be set aside as a mere mistake, as Ross suggests (p. 88). The treatment of the Forms as distinct entities even seems to necessitate this "mistake": we can think of Beauty as a distinct thing only if we think of it as a purified and perfected *beautiful*.

In line with his interpretation of the Forms simply as universals, Ross says (p. 24) that "it is not by despising the senses and turning to pure contemplation, but by using the senses and finding out what they suggest to us, that (in Plato's view) we arrive at a knowledge of the Ideas". Now this is a fair account of Plato's description of anamnesis, but it is utterly at variance with what is said elsewhere in the

¹ I owe this phrase to Professor John Anderson.

Phaedo (e.g., 65e-66a: μήτε τιν' ὄψιν παρατιθέμενος ἐν τῷ διανοεῖσθαι μήτε ἄλλην αἴσθησιν ἐφέλκων μηδεμίαν). And the latter view, that thought can operate entirely by itself, is essential for the argument; on it rest both the description of philosophy as a "practising to die", and the very possibility of giving a description (in terms we can understand in this life) of the life after death. This reveals another conflict within Plato's theory: the Forms both enter into ordinary observation and are objects of pure thought.

It must be remembered that this plurality of roles is the strength, as well as the weakness, of the theory of Forms; an implication of the theory which has appealed to many later Platonists (such as Galileo) is that *the objects of science are in some way ideals*.

Similarly, the theory seems to have had its source not just in "common names", but particularly in situations where there is some *difficulty* about a common name. Admitting that it starts from Socrates' search for definitions, we must remember that Socrates looked for definitions not of terms like "stone" (where one could say without hesitation, "This is a stone") but of moral and aesthetic terms, where it is hard to see that a number of acts are all, say, *intrinsically* courageous in the same sense (while other, somewhat similar, ones are not) and where relational or partly relational accounts of these terms, such as those advanced by the sophists, have a good deal of plausibility.

Indeed, while Plato's defenders and critics alike have usually treated the theory of Forms as a theory of predication, it might be at least as accurate to describe it as a theory of the apparent failures of predication; it is precisely where there is some obstacle to simple unequivocal predication that the Forms are introduced.

Plato, indeed, lumps together a great variety of obstacles which ought to be overcome in different ways. Forms are brought in where there are relative or, as we might say, scalar terms ("hot", "tall", "strong", "like", etc., where we cannot

say simply "Simmiās is tall" or "Simmiās is like"); also where the particulars are inexact (equality and geometrical terms generally are the clearest cases, but moral and aesthetic terms also come under this head); also where the particulars "appear" variously (now beautiful, now ugly, equal to one man, unequal to another); and where the particulars are inconstant. Examples of the introduction of Forms on these grounds are found in *Phaedo* 74-9 and 102, and *Parmenides* 129. Ross notes it as an inconsistency in Plato that in the *Phaedo* he fails to recognise that "things which seem to some person unequal may nevertheless be . . . perfect examples of equality", but this is at any rate one important line of thought in Plato, exemplified again in the argument (*Republic* 476-9, especially 479a) that the object of opinion must be between being and not-being.

In the *Republic* (523-4), while discussing the studies which "draw the soul towards reality (οὐσία)", Socrates makes the very point that I am suggesting. The mind is not roused to reflection by perceptions which are "judged adequately by sense", but by those with regard to which "sense does nothing sound". That each finger is a finger does not excite thought, but the difficulties of deciding whether a finger is large or small, and of dealing with the sense perceptions that the same thing is both hard and soft, etc., do excite thought which leads ultimately to the Forms.

This treatment throws light on the passage (130b-e) in the *Parmenides* where Socrates is sure that there are Forms of the just, the beautiful, the good, and "everything like that", doubtful whether there are Forms of man, fire, and water, and almost certain that there are not Forms of mud, hair, and dirt, since "in these cases the things are just as we see them" (ταῦτα μὲν γε ὥπερ ὁρῶμεν, ταῦτα καὶ εἶναι). The point is not only that muddiness is not an ideal,² but also that mud just *is* mud,

² Cf. Anderson's criticism of the Socratic view in "Realism and Some of its Critics", *A.J.P.P.*, Vol. VIII, No. 2, pp. 124-5: ". . . the ridiculousness of saying . . . that a particular piece of mud is striving after the ideal of perfect muddiness". Yet "whether we take the case of mud or any other . . . we have to admit that they are just something".

that there is no difficulty about so describing the perceived object and so no need to introduce a Form. The cases of man, fire, and water are doubtful, because on the one hand Socrates, etc., just *are* men, and yet there is some question of a distinction between those who are more or less perfect as men, and similarly fire or water can be more or less pure.

It is implausible to say that the theory of Forms was developed simply to explain predication. For what need would there have been to *explain* it, to go beyond the fact of predication itself, if it had not seemed to be in some way faulty? We must here reject the now-popular view (not, of course, shared by Ross) that the theory originates from a misunderstanding of the use of abstract nouns, from an attempt to find things of which "beauty", "unity", etc., are names; for such nouns were not much used before Plato, and he had to coin many of them, and use awkward circumlocutions instead of abstract nouns as names of the Forms—a clear case of the philosophical theory *preceding* the linguistic forms from which it is supposed to have arisen!

In his final chapter Ross discusses Plato's views on the question whether the Forms are immanent or transcendent. He divides (p. 228) the words used to express the relation between Forms and particulars into "a group . . . implying or suggesting the immanence of the Forms, and a group implying or suggesting their transcendence", lists the occurrences of these in the dialogues in order of composition, and draws the conclusion that "there is a general movement away from immanence towards transcendence. In the early period almost everything speaks of immanence". However, the contrast between immanence and transcendence misses the vital point; that even on the "immanent" view the Forms are not merely "different from sensible things" (p. 227) but distinct from the whole observable situation: the fact that beauty is present in the man is distinct from (and explains, or explains away) the fact that the man is beautiful. But besides this, Ross's study of the words used is misleading, for several reasons.

(i) This "relation" is rather rarely mentioned in any way in the later dialogues (except the *Timaeus*), and in the later dialogues other than the *Timaeus* there is a big majority of "words suggesting immanence". (ii) The "words suggesting immanence" (*in, possess, receive, participate, etc.*) only suggest that a Form *can* be present in a particular: they leave it open that the Form may also exist separately, and indeed in the *Phaedo* (103b5) Socrates speaks at once of the Form "in us" and "in nature". On the other hand the "words suggesting transcendence" (*pattern, αὐτὸ καθ' αὐτό, strive after, imitate, etc.*) suggest that the Forms *must* be transcendent. (iii) The choice of words is really determined by Plato's purpose from time to time rather than by any change in his views; thus in *Phaedo* 100-6, apart from one αὐτὸ καθ' αὐτό, there are only "words suggesting immanence", because Socrates is there discussing the effect of the Forms on the particulars—a thing is beautiful because it participates in Beauty, etc.—whereas in the same dialogue in 74-8 there is nothing but "words suggesting transcendence", because the subject there is the nature of the Forms themselves and their distinctness from particulars. Thus the context itself would suffice to explain the occurrence only of "words suggesting immanence" in the earliest dialogues, even if Plato already held a "transcendent" view.

Ross's conclusion (p. 231) is that while Plato was not satisfied with either "participation" or "imitation" as an account of the relation between particulars and Forms, "he saw no way of getting nearer to the truth than by using both expressions"; they were "complementary metaphors", and he "may even have had an inkling of the fact that the relation is completely unique and indefinable". Ross is intending to reduce this relation simply to predication, but while we might say that one of the lessons of the *Parmenides* is that predication is not a *relation* at all, the essential point for the interpretation of Plato is that the Form is always something more than a predicate.

There are two minor points concerned with the "mathematics". (i) Ross's suggestion (pp. 223-4) that Plato finally rejected these as mathematical fictions, and that this might even be part of the meaning of the statement in the *Republic* that dialectic annuls the hypotheses of mathematics, will not harmonise with his earlier statement (p. 62) that "it was not till very near the end of his life that Plato formulated the doctrine, though he had long been on the point of formulating it". In any case there is no need to suppose, as Ross does, that Plato's rejection of points is part of a general rejection of mathematics; the paradoxes of Zeno, to which Plato adopted the "indivisible lines" solution, would be a sufficient reason for his rejection of points by themselves. (ii) Ross seems to be wrong in taking *αὐτὰ τὰ ἴσα* (*Phaedo*, 74c1) as a foreshadowing of the mathematics (p. 60). This phrase, though *αὐτὸ τὸ ἴσον* is commoner, is just another parallel to *αὐτὸ τὸ δίκαιον*, not to a possible *αὐτὰ τὰ δίκαια*; the plural refers not to a plurality of perfect instances of equality, but to the fact that equality, even perfect equality, requires a situation with at least two components. So this phrase is no more than another illustration of the frequent treatment of the Form as a perfect particular.

My general impression, then, is that Sir David Ross is too much of an Aristotelian in spirit to be quite fair to Plato; his estimate of the broad character of the theory of Forms is less balanced than his discussion of detailed points. He defends Plato, indeed, against Aristotle's criticisms (pp. 226, 233), but at the price of minimising the differences between them.

J. L. MACKIE.

REVIEWS

METHODS OF LOGIC. By W. V. Quine. (Routledge and Kegan Paul, 1952. xxii + 264 pp.) Price (U.K.), 16s.

The time has now come when the teacher of modern logic has several first-class elementary text-books on the subject from which he can take his pick—text-books, I mean, that are also classics in their own right (like, on the traditional side, Whately's *Elements* or Jevons's *Elementary Lessons*). He has, for example, Tarski's *Introduction to Logic*, Bochenski's *Précis*, and now this of Quine's (which first appeared, in an American edition, in 1950). What are the strong and the weak points of Quine?

I'll take the latter first. (1) Only a limited part of the field is dealt with, though certainly it is the most fundamental part—the logic of propositions (in its orthodox, two-valued, non-modal form, and without “variable functors”), the logic of one-place predicates (i.e., in the language of *Principia Mathematica*, propositional functions of a single argument), and the logic of predicates generally; with, at the end, a formally sketchy though philosophically rich discussion of singular terms, descriptions, identity, classes, relations in extension, the logical foundations of arithmetic, and the theory of types. There is nothing about, for example, the use of interpretative “models” in critically handling deductive systems, as so lucidly expounded and illustrated in Tarski; and syllogistic logic is treated (with the aid of Venn's diagrams) as simply a crude precursor of the modern logic of predicates, more recent “axiomatisations” of it being ignored. (2) Only the Peano-Russellian type of symbolism is employed throughout; that of Lukasiewicz is not presented even as an alternative.

And now for the merits of the book. (1) It does not need (as, for example, Bochenski does) to be accompanied by oral teaching; and I have not seen any better book for putting in the hands of an educated outsider whom one does not expect ever to darken the doors of a Logic classroom (unless he be a teacher of mathematics, in which case Tarski is probably his meat). (2) It is full of shrewd dodges and wise practical advice for the working out of concrete logical problems, both at the level of putting standard English into “logical form” (both the old-style A, E, I and O and the new-style “There is something x such that, etc.”), and at the level of manipulating the pure symbolism. There is, for example, an admirable method for calculating the possible truth-values of a formula with

a large number of propositional variables; and a way of setting out derivations which not merely proceed from one logical law to another, but rather work out the logical consequences of formulae which may not themselves express logical laws at all. (This, of course, has a subsidiary use in pure logic, since one way, and indeed the most natural way, of showing that "If α then β " is a logical law is to show that β is a logical consequence of α .) The dodges, moreover, are always accompanied by a clear explanation of why they work, in the course of which one learns much about the pure theory of the subject. (3) Throughout the book, but especially in the Introduction and in the last part (on classes, descriptions, etc.), there are lucid and provocative discussions of the general philosophical issues suggested by the symbolic procedure, *e.g.* (in the Introduction) the reason for our certainty of logical laws, and (in the last part) how far we can get in logic and mathematics without supposing that abstract nouns are names of abstract entities.

The method for calculating truth-values, though it seems only common sense once you've been told about it, is too good not to pass on; but I'll vary it a little by using the Polish notation. Consider, for example, the proposition $CCApqKrsCKpqArs$. (If either- p -or- q implies both- r -and- s , then both- p -and- q implies either- r -or- s .) The truth of this is probably evident enough intuitively; but if we wished to verify it by truth-tables we would need to consider successively the 16 possible combinations of truth-values of p , q , r and s , and have 16 lines of calculation like

$$CCA11K11CK11A11 = CC11C11 = C11 = 1$$

$$CCA11K10CK11A10 = CC10C11 = C01 = 1$$

etc. But Quine would do it like this (the numbers in brackets indicate the order of the steps):

$$CCApqKrsCKpqArs$$

- | | |
|-----------------------|------------------------|
| (i) $CCA1qKrsCK1qArs$ | (xi) $CCA0qKrsCK0qArs$ |
| (ii) $CC1KrsCqArs$ | (xii) $CCqKrsC0Ars$ |
| (iii) $CKrsCqArs$ | (xiii) $CCqKrs1$ |
| (iv) $CK1sCqA1s$ | (viii) $CK0sCqA0s$ |
| (v) $CsCq1$ | (ix) $C0Cqs$ |
| (vi) $Cs1$ | (x) 1 |
| (vii) 1 | |

Here we begin by considering (i) the case in which $p = 1$. The step from (i) to (ii) depends on the facts that $A1q = 1$ (for a disjunction is always true if one of its members is) and $K1q = q$ (for the conjunction of a true proposition with another proposition will be true if that other proposition is true, and false if it is false). The step from (ii) to (iii) depends on the fact that $C1a = a$ ($C11 = 1$, $C10 = 0$).

We then consider in succession the sub-possibilities in which $r = 1$ and $r = 0$ (r rather than q because at this stage r appears more often, and so more variables are eliminated at one stroke). The step from (iv) to (v) is like that from (i) to (ii). That from (v) to (vi), and also that from (vi) to (vii), use the equation $Ca1 = 1$ (a material implication with a true consequent is true). Turning now, in (viii) to the sub-possibility in which $r = 0$, the step to (ix) uses the equations $K0s = 0$ (a conjunction with a false member is false) and $A0s = s$ (a disjunction with a false member has the truth-value of its remaining member). The step from (ix) to (x) uses $C0a = 1$ (a material implication with a false antecedent is true). We then consider, in (xi) to (xiv), the case in which $p = 0$; the grounds of the steps here taken should by now be obvious. All possibilities having now been covered, the conclusion is that the formula yields a true proposition no matter what propositions the variables 'p', 'q', 'r' and 's' may stand for, and so expresses a logical law.

Still further abbreviation of truth-value calculations is possible by a method evolved (since Quine's book first appeared) by an English Dominican, Ivo Thomas; but to work by this method one has to have a couple of pages of specially constructed tables beside one. The tables, with their *rationale* and instructions for their use, may be found in *Dominican Studies*, Vol. IV (1951), pp. 72 ff. This branch of logic, one might say, has now reached the stage of using something like logarithms. But if you've lent or lost your copy of *Dominican Studies*, Quine's rules of thumb will get you there quickly enough.

A. N. PRIOR.

THE HABITUAL CRIMINAL. By Norval Morris. (London School of Economics: Longmans, Green & Co., 1951. 395 pp.) Price (U.K.), 27s. 6d.; (Aust.), 45s. 9d.

This book, whose author is Senior Lecturer in Law in the University of Melbourne, undertakes a comparative survey of the history and present status of the legislation in Britain, the British Commonwealth and some European countries (but unfortunately not that in the U.S.A.) under which certain "dangerous" persistent offenders may be declared "habitual criminals" and subjected to preventive detention for an indeterminate period either (as now in Britain) instead of, or (usually) in addition to, a punitive term of imprisonment. Its critical treatment of the legislation, which is most detailed for Britain and includes a complete chapter on the relevant clauses of the 1948 Criminal Justice Act, makes it of special value to lawyers and criminologists. (I have to report, however, at least one serious error—through a misquotation Dr. Morris falsely credits the N.S.W.

law with ensuring that no one can be declared an habitual criminal for any number of offences against property only (p. 88).) But it also raises incidentally a number of problems of interest to philosophers.

"Preventive detainees are, by definition, the failures of the community's reformatory techniques" (p. 241). Consequently "reformation is not a significant aim of preventive detention"—though it may be a "by-product of time and the preventive detention regime" (p. 243, footnote 2). The habitual criminal's detention beyond the period of any punitive sentence is primarily, if not solely, for the purpose of protecting society, and would thus seem to be a counsel of despair. Two difficulties arise: (1) If the habitual criminal has proved incorrigible by any known methods, and no new reformatory techniques are to be tried during his detention, what reason is there to expect any change in him which will enable him to be released without serious danger to society? The only suggestion of an answer that I can find in Dr. Morris's book is that reformation may come "by a process of 'ageing'" (p. 241, footnote 1). Even if this does happen, it must be hard to verify before release. On Dr. Morris's own admission, the main evidence for determining that a prisoner is ready for release is his prison record. But, as he goes on to point out (p. 229), "the success of the prisoner in conforming to the requirements of prison discipline is useless" as evidence of his ability to assume the responsibilities of free citizenship. And this is not all. A prisoner who has turned "bad" from the point of view of the prison authorities by challenging prison discipline "might well be at the correct psychological stage for successful discharge" (p. 230, footnote 2).

Various forms of partial, experimental release are possible in order to determine, without too much danger to the public, whether "ageing" (or some other mysterious factor) has effected the necessary change. The one most commonly used is conditional discharge on licence. Dr. Morris believes (p. 231) that this places too great a strain on the prisoner, since it pitchforks him too suddenly from the shelter of prison, where he has no decisions to make, to the responsibilities and temptations of freedom. He himself advocates (pp. 231-4) a brief period of leave from prison during which the man, while still technically a prisoner in charge of the prison governor, will work with a co-operative employer not far from the prison, who will regularly report on his behaviour. The prisoner would receive the same wage and treatment as other employees and would pay for his own accommodation, if necessary in the prison itself. Only if this experiment was successful would he be released on licence.

While this proposal is well worth a trial, neither it nor any other scheme of the kind disposes of my objection to any method for terminating preventive detention, that it must work blindly—a fact

which throws doubt on the justice of such indeterminate sentences. On Dr. Morris's own admission, the authority determining release would have little more than "hunch" to rely on for deciding when a prisoner was ready for such an experiment.

(2) If preventive detention cannot be reformatory, neither should it be punitive. Segregation of the "habitual criminal" should be the sole aim, and he should be detained under conditions markedly different from those of penal servitude. *Some* element of punishment cannot of course be avoided, since it is involved in deprivation of liberty under any conditions. This is a strong argument for the British decision to make preventive detention replace, instead of following, a punitive sentence for the last offence.

Dr. Morris points out that in so far as preventive detention is punishment, the maxim "*nulla poena sine lege*" is broken by the wide administrative discretion exercised over the detainee. But he believes that, in spite of the punishment involved in all detention, it is possible to devise conditions of segregation which will not be essentially punitive, so that the maxim will not be infringed.

However this may be, his own survey shows that in most countries the habitual criminal is in fact being punished by an indefinite continuation of his prison sentence, added on to, and served under much the same conditions as, a term of penal servitude imposed as punishment for his last offence. Not only is the "*nulla poena*" maxim broken, but, what is more serious, he is being unjustly punished, since he will have already served his due penal sentence. In Australia (with the exception of Western Australia) there are no special buildings for preventive detainees, and apart from some minor privileges in the later stages of detention their treatment differs little from that of an ordinary prisoner. This criticism still stands, even though we may fully agree that "minor" alleviations of the normal prison routine are of vital significance to the prisoner himself" (p. 241).

In an introductory chapter on theories of punishment, Dr. Morris connects the move away from a retributive theory to utilitarian theories with a growing adherence to determinism. But it is doubtful whether the popular acceptance of a utilitarian approach has in fact been accompanied by a popular belief in determinism, though it has been connected with a waning of the belief in *sin*. Dr. Morris suggests that the conception of moral culpability involves the acceptance of "an indeterminate freedom of will" (p. 17) and the treatment of each offence as an isolable event, the result of a single act of choice. But even a deterrent theory of punishment presupposes that it is possible to choose between alternatives, since it offers fear of the consequences

as a motive for choosing not to break the law, and on the other hand it is an argument *against* indeterminism that it fails to account for responsibility and especially for degrees of responsibility.

Dr. Morris argues (against what is commonly believed by "the wise" as well as "the many") that deterrence and reformation are not incompatible. "Deterrence" can of course mean either deterrence of potential offenders (that is, all of us), through fear of the consequences as demonstrated by the punishment of actual offenders; or deterrence of actual offenders from offending again, through fear of further punishment. Both of these, but especially the former, are held to conflict with reformation, on the ground that the kind of treatment likely to reform does not hold the same terrors, and therefore has not as strong a deterrent effect as punishment which merely punishes.

Dr. Morris's reply to this is not as clear as it might be, because he fails to distinguish between the possible reformative effect of the strictly punitive measures and the effect of non-punitive measures of "rehabilitation" which imprisonment makes possible. One of his arguments is that reformative techniques, because they involve "individualisation of punishment", have an unpredictable quality which, as such, is specially deterrent (p. 12). I doubt if fear of the unknown is frightening if you have good ground for believing it to be more tolerable than the known. But there is another reason in favour of Dr. Morris's conclusion which he does not mention. The ordinary citizen regards "going to prison" as bad enough in itself and in the main neither knows nor cares (as prison reformers have reason to lament) about the details of what actually goes on in prisons. There would seem to be a stronger case for the less popular view that there is a conflict between the ends of reformation and deterrence in the case of those who *have* offended, that reformative measures which fail to reform also fail to deter.

A. K. STOUT.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

THE NATURE OF HISTORICAL EXPLANATION. By Patrick Gardiner. (Oxford University Press, 1952. xii + 142 pp.) Price (Aust.), 19s. 6d.

THE PHILOSOPHY OF ARISTOTLE. By D. J. Allan. (Home University Library, O.U.P., 1952. 220 pp.) Price (Aust.), 7s. 6d.

THE PSYCHOLOGY OF RELIGION. By L. W. Grensted. (H.U.L., O.U.P., 1952. vi + 181 pp.) Price (Aust.), 7s. 6d.

A HISTORY OF FREEDOM OF THOUGHT. By J. B. Bury; with an Epilogue by H. J. Blackham. (H.U.L., O.U.P., 1952. 246 pp.) Price (Aust.), 7s. 6d.

The second edition of a standard work first published in 1913.

PHILOSOPHICAL ESSAYS. By Isaac Husik. Edited by Milton C. Nahm and Leo Strauss. (Basil Blackwell, Oxford, 1952. xlii + 358 pp.) Price (U.K.), 35s.

Essays by a Jewish philosopher on Judaism, Jewish Philosophy, Aristotelianism, and Legal Philosophy.

ROGER BACON AND HIS SEARCH FOR A UNIVERSAL SCIENCE. By Stewart C. Easton. (Basil Blackwell, 1952. vii + 255 pp.) Price (U.K.), 25s.

SPINOZA IN SOVIET PHILOSOPHY. Essays selected and translated with an introduction by George L. Kline. (Routledge, 1952. vii + 190 pp.) Price (U.K.), 25s.

HISTORY OF RUSSIAN PHILOSOPHY. By N. O. Lossky. (Allen and Unwin, 1952. 416 pp.) Price (U.K.), 30s.

THE LOGIC OF PERSONALITY. By Bernard Mayo. (Jonathan Cape, 1952. 188 pp.) Price (U.K.), 10s. 6d.

SEMANTICS AND THE PHILOSOPHY OF LANGUAGE. Edited by Leonard Linsky. (University of Illinois Press, 1952. ix + 289 pp.) Price, \$3.75.

A collection of essays (previously published in periodicals) on topics connected with meaning, by Tarski, C. I. Lewis, Quine, Russell, Carnap, and others.

PRIMITIVE MAN AND HIS WORLD PICTURE. By Wilhelm Koppers. Translated by Edith Raybould. (Sheed and Ward, 1952. viii + 264 pp.) Price (U.K.), 16s.

THE LITTLE WORLD OF MAN. By J. B. Bamborough. (Longmans, 1952. 187 pp.) Price (Aust.), 33s. 6d.

An account of Renaissance psychological theory, fully illustrated with extracts from contemporary writers.

BOOKS RECEIVED.

- LUCIFER AND PROMETHEUS. By R. J. Zwi Werblowski; with a preface by C. G. Jung. (Routledge, 1952. xix + 120 pp.) Price (U.K.), 16s. A study of Milton's Satan.
- RELIGION IN THE MODERN WORLD. Essays by Viscount Samuel, R. H. Thouless, Rev. T. Corbishley, Canon Raven, Radhakrishnan, Very Rev. W. R. Matthews, and Sir Wilfred Garrett. (Allen and Unwin, 1952. 110 pp.) Price (U.K.), 7s. 6d.
- RELIGIOUS FAITH, LANGUAGE, AND KNOWLEDGE. (A philosophical preface to theology.) By Ben F. Kimpel. (Philosophical Library, 1952. x + 162 pp.) Price, \$2.75.
- OF GOD, THE DEVIL, AND THE JEWS. By Dagobert D. Runes. (Philosophical Library, 1952. 186 pp.) Price, \$3.00.
- SONGS OF ZARATHUSTRA. Translated by Dastur F. A. Bode and Piloo Nanavutty. (Allen and Unwin, 1952. 127 pp.) Price (U.K.), 8s. 6d.
- THE ENGLISH PRISON AND BORSTAL SYSTEMS. By Lionel W. Fox. (Routledge, 1952. xiii + 479 pp.) Price (U.K.), 30s.
- PRACTICAL PSYCHOLOGY. By F. K. Berrien. (Macmillan, Revised edition, 1952. ix + 640 pp.) Price, \$5.00.
- EMPIRICAL FOUNDATIONS OF PSYCHOLOGY. By N. H. Pronko and J. W. Bowles. (Routledge, 1952. xvi + 464 pp.) Price (U.K.), 25s.
- THE PHILOSOPHY OF WILHELM DILTHEY. By H. A. Hodges. (Routledge, 1952. xxvi + 368 pp.) Price (U.K.), 28s.
-

ERRATUM.

Volume XXX, No. 1, page 57: In the fourth line, for "indirect" read "direct".

NOTES AND NEWS

AUSTRALASIAN ASSOCIATION OF PSYCHOLOGY AND PHILOSOPHY.

Thirtieth Annual General Meeting.

The Congress and Annual General Meeting for 1952 were held at Canberra from August 16 to 20, 1952. The programme was as follows:

Saturday, August 16:

8 p.m.—“Equality”, Professor A. Boyce Gibson.

Sunday, August 17:

2.30 p.m.—“Remembering”, Mr. Bruce Benjamin.

8 p.m.—“Metaphysics as the Search for Inclusive Contrasts”,
Professor Charles Hartshorne (University of Chicago).

Monday, August 18:

10.30 a.m.—“Scientific Entities”, Mr. J. B. Thornton.

2.30 p.m.—Council Meeting.

7.30 p.m.—Annual General Meeting.

8 p.m.—Presidential Address: Mr. S. A. Grave, “Butler’s
Analogy”.

Tuesday, August 19:

10.30 a.m.—“Stuff”, Mr. D. Gunner.

8 p.m.—“Can Ethics be Studied?”, Professor John Anderson.

Wednesday, August 20:

10.30 a.m.—“Physics and Reality”, Mr. John Clendinnen.
